while collecting in the region of perpetual snow on James' Peak, I found and preserved a flower of *Trollius laxus* in which the stamens and most of the pistils were transformed into petals. These petals were of the same color as the sepals, but shorter, though broad, and in other respects finely developed. The plant is abundant in the high mountains of this latitude. — Rev. E. L. GREENE, *Greeley, Colorado Territory.*

ZOOLOGY.

THE LAST OF "BONASA JOBSII."— The immediate witnesses of the sudden appearance and prompt destruction of the above mentioned bird, greatly regret that a page of the NATURALIST should be occupied in recording its career; but since this is the case (see issue of March, p. 172) and since some of the statements of your correspondent require correction, we hope that a final shot may be permitted; and first since neither the NATURALIST nor its correspondent appears to have seen the following communication in which the describer of "Bonasa Jobsii" cheerfully resigned his first-born and since it contains some instructive remarks from high ornithological authorities, we request its insertion here.

"I beg the privilege of reading the views of two distinguished ornithologists, Professor S. F. Baird and Dr. Elliott Coues, upon the article published in THE ERA of December 8th, entitled "Bonasa Jobsii."

Professor Baird writes: 'Your letter of the 11th inst. with the accompanying description of the supposed new species of grouse has just been received, and after a careful examination of the account I have no hesitation in pronouncing your bird to be one of the numerous variations assumed by the common ruffed grouse. A difference in shape, or color of the plumage, or a discrepancy in an observed measure, or an anatomical peculiarity from a description based upon a dried specimen, is of comparatively little moment. The number of tail-feathers as a criterion is equally illusive, when the normal average of twelve is exceeded. When I published my work on the Birds of North America I was in what might be called the analytical stage of natural history development. My present condition is synthetical. I take more pains now to subordinate forms, once considered specific, than I do to establish them as such. I admit now but one species of *Bonasa* in the United States, with, however, an Eastern race, a Middle and a Western, these diversified by differences of plumage and slight variations of proportions which, however, are of no great

importance. A great difference in the colors of the Eastern bird has been frequently met with, and we have several specimens in our collection, answering very well to the account given of your bird.'

Dr. Coues, writing to the editors of THE ERA, says: 'The interest I take in ornithology is my excuse for begging enough of your valuable space to correct an unfortunate error that appears in your issue of December 8th. I say unfortunate, because the article in which a supposed new species of bird, *Bonasa Jobsii* is characterized, merely adds to the synonymy with which ornithology is overburdened. So far from conflicting with the genus *Bonasa*, or forming a new genus, the ruffed grouse that Mr. Jobs was unlucky enough to shoot, and Mr. Jaycox still more to write about, shows nothing of specific consequence. The number of rectrices of *Bonasa umbellus* varies from sixteen to twenty. (Vid., e. g., Bull. Mus. Comp. Zoöl., ii, 205.) The relative proportions of the tarso-metatarsus and medius-dactyl sometimes differ more than they appear to in this instance, while the discrepancies in color that are adduced by the writer are strictly within the range of individual variation.'

Thus we see Professor Baird and Dr. Coues agree that the bird described was a ruffed grouse.

There was one very important sentence omitted in the published article in THE ERA. It is: 'As I have no specimen of ruffed grouse I shall have to note the differences between the descriptions of such authors as I can consult and the bird in question.' The authorities I had the opportunity of studying were Baird's 'Birds of North America,' Wilson's 'American Ornithology,' Samuel's 'Birds of New England' and 'Observations of Wilson's American Ornithology;' in none of the above works could I find such a description of the ruffed grouse as would answer to the grouse in question. I could not find any remark which would lead one to think that the rectrices of the genus *Bonasa* were ever more or less than eighteen, and in reading Baird's 'Birds of North America' I found so many different species of birds differing from each other in so slight a degree that it led me to suppose the bird was one that had not been described.

In Professor Baird's letter he says: 'I admit now but one species of *Bonasa* in the United States.' At the time of the the publication of his work he describes two — *Bonasa umbellus* and *Bonasa Sabinii;* these he now believes to be the same bird, the difference being caused by geographical variation. Thus, by having a more complete knowledge of the different genera, he has changed his opinion in regard to the genus *Bonasa.* I do not doubt that, had Baird possessed the specimen I described, at the time he was writing 'Birds of North America,' he would have formed another genus for it; at least it would seem so from the manner in which he has made new species of specimens which

differ from each other in so few characteristics."—*From report of Proc. C. U. Nat. Hist. Soc., Jan.* 13, 1872. *Cornell Era, Jan.* 19.

Your correspondent does not allow for the immense change that has been wrought in science within the last ten years, and forgets that while a young student may be well versed in the standard works of twenty years ago, in which many species rest upon less basis than "Bonasa Jobsii;" he would not, unless directed, be apt to see the recent periodical publications in which as yet appear the only evidences of the great and beneficial revolution from the "analytical" to the "synthetical" stage of science; and so the charge of "knowing nothing of ornithology" seems to us not only harsh but unwarranted. However, as Mr. Jaycox has so readily and cheerfully acknowledged his ornithological blunder, "Bonasa Jobsii" may be regarded as the name of an extinct species, and as laying no claim to synonymy with Bonasa umbellus, and we have only to comment upon some parts of the note which occasioned this communication.

The "Cornell Era" is *not* a "publication of an institution of learning" in any sense which entitles its contents to more consideration than other "newspaper science;" it is published under the sole direction of five students and it differs from other college periodicals mainly in its willingness to publish the proceedings of the various scientific societies. So neither the Era nor the University are in any way responsible for "Bonasa Jobsii" but the Natural History Society, from the proceedings of which the description was an extract, and by the President of which (and not by the President of the University as inferred by your correspondent) the specific name was suggested; and since some of your readers may know that the Professors of the University are honorary members of the Society, and often attend its meetings, it is but fair to them to state that none were present when "Bonasa Jobsii" was introduced; had they been, Mr. Jaycox would have been at once referred to the able papers of Mr. Allen (Bull. Mus. Comp. Zoöl. vol. i, No. 8, and vol. ii, No. 3) in which the whole subject of specific characters is discussed.

In conclusion I must allude to the contrast between the manner of your correspondent's criticism and those of Prof. Baird and Dr. Coues, who may fairly be regarded as exponents of the older and younger American ornithologists. Their notes are critical without being sarcastic; and they evince respect for the honest

though misdirected zeal of a young naturalist, instead of commiseration for an "ornithological blunder," and they include nothing that might not properly be said in the presence of the person criticised, and, now as "Bonasa Jobsii" is a thing of the past let us hope to hear no more of it, and that any reply to the foregoing will be confined to the question of scientific ethics and the limits of kindly criticism.—BURT S. WILDER, *Cornell University.*

ORNITHOLOGICAL BLUNDERS.—In the March number of the NATURALIST occur some remarks respecting "An Ornithological Blunder." Such a gross mistake as that made in the case of "*Bonasa Jobsii*" certainly demanded vigorous criticism; yet are there not palliating circumstances attending this "blunder" that render the language of your correspondent's critique unnecessarily harsh? To me it seems that there are. Unquestionably bad as was the work your correspondent was called to pass upon the indiscretion here alluded to was evidently encouraged, if not indeed actually induced, by equally unfortunate "blunders" previously made, not by mere tyros, but by recognized ornithological authorities. That this was the case seems evident from the comparisons and precedents cited in the remarks accompanying the description of *Bonasa Jobsii.* Is not, in fact, *Bonasa Jobsii* one of the legitimate fruits of the excessively analytic system followed in the only general works on North American ornithology accessible to students? The authors of these valuable works may have modified their opinions, and even their methods of working since the publication of those works, but as yet the general student has no means of knowing it. It seems to me that as long as species no more worthy of recognition than *Bonasa Jobsii* have the appearance of being currently accepted, because not yet publicly retracted, mistakes like that made by Mr. Jaycox need not be looked upon as wholly unpardonable. In fact if the author of *B. Jobsii* could have truthfully added, *Hab.* "Columbia River," or, "Hudson's Bay Territory," to his description, his pseudo-species might even now have been less summarily dealt with though none the less untenable. By these remarks, however, I do not by any means wish to encourage such kind of work, but merely desire to call attention to the fact that in Mr. Jaycox's case there are extenuating circumstances.—†††.

[We gladly make room for the above, from Prof. Wilder and

another esteemed contributor, with the remark that "our correspondent" who ranks with the best ornithologists of the day, performed his duty with the utmost good will towards the author of the new name, and was simply severe on the principle, and not on the author, as a warning to all young naturalists not to run headlong into print.—EDITORS.]

VIBRATIONS OF THE TAIL IN SNAKES.—Professor Shaler has a note on this subject in the Am. Naturalist, Jan. 1872, p. 35. In 1849 Professor S. F. Baird placed in my hands for translation, the invertebrate zoology of Heck's Iconographic Encyc., but finding it a poor compilation, I rewrote it, merely following the plates of the German edition. I incorporated various original observations of my own and the following passage occurs on p. 6-7 of *zoology*.

"Instinctive actions are not taught, although a permanent habit may become an instinct. The young duck swims at once, the young snapping turtle bites when taken from the egg, and a harmless serpent without fang or rattle will vibrate its tail like a rattlesnake producing a similar sound among dry leaves. The brain of the young is modelled upon that of the adult, and where the scale of ideas is limited, they must be as essentially hereditary as the external form. (In a note—These views are favorable to the doctrine of innate ideas, which is generally opposed by speculative reasoners.) The brain of the young is not necessarily that of the adult, but that of the adult at an earlier stage. So a quality or habit is not always transmissible from a parent to its immediate offspring, but it may appear in a more distant descendant, by a kind of alternation of generations."

Professor Cope (Proc. Am. Phil. Soc. July–Dec. 1871, p. 248,) mentions several poisonous and harmless snakes which vibrate the tail when excited—a phenomenon I observed in 1841.

A point in the extract affords an illustration of the ignorance of unscientific people, and their inability even to read scientific matter correctly. I had asserted that "the young snapping turtle bites when taken from the egg,"—I might have stated the fact a little stronger, for if touched, the unborn snapper turns its head and opens its mouth when the shell is broken sufficiently to expose the head. Seemingly on this statement, the "Atlantic Monthly" (May 1860, p. 516) asserts that—"Agassiz, who, knowing the savage snap of one of the full-grown *Testudinata*, is said to have asserted that, under the microscope [!] he has seen the juvenile turtle snapping precociously *in embryo*."—S. S. HALDEMAN, *Columbia, Pa.*

[Agassiz's original statement about the young turtle is in his Contr. Nat. Hist. U. S., i, p. 175 (1857) and is as follows:—In speaking of the "Succession of Characters" he says "The Snapping Turtle, for instance, exhibits its small crosslike sternum, its long tail, its ferocious habits, even before it leaves the egg, before it breathes through lungs, before its derm is ossified to form a bony shield, etc.; may, it snaps with its gaping jaws at any thing brought near, though it be still surrounded by its amnios and allantois, and its yolk still exceeds in bulk its whole body." In a foot note he further adds, "Pr. M. v. New-Wied quotes as a remarkable fact, that the Chelonara serpentina bites as soon as it is hatched. I have seen it snapping in the same fierce manner as it does when full-grown, at a time it was still a pale, almost colorless embryo, wrapped up in its fœtal envelopes, with a yolk larger than itself hanging from its sternum, three months before hatching."—EDITORS.]

THE AFFINITIES OF CRINOIDS.—Metschnikoff, to whom we owe so many embryological investigations, has published preliminary notices* of the early stages of Comatula which are of the utmost importance, as they throw an entirely new light on the affinities of the Crinoids. Thoroughly familiar with the pluteus of Holothurians, Echini, Starfishes and Ophiurans, he commenced the investigations of their earlier stages with the determination of tracing the presence of the peculiar water system of the larvæ of the other orders of Echinoderms; what had been previously written by Busch, Allman and Thomson, on the early stages of Comatula, giving no date whatever bearing upon the subject.

To his surprise he found no such water system, nor could he trace anything in any way homologous to it; he also discovered that what constitutes the water-system of adult Crinoids, which has always been homologised with the water-system of other Echinoderms is developed in a totally different manner. In the free swimming Comatula larva the bag-like digestive sac is the only organ developed, it becomes the digestive cavity of the adult after the larva attaches itself to the ground. He noticed the tentacles as diverticula of the digestive sac in the interior of the larva; these subsequently force their way through to the exterior, at the time when the digestive bag has become further differentiated, and is provided with a mouth opening in the centre of the oval disk, and an anus opening not far from it on the side of the calyx. There

*Bulletin Acad. St. Petersburg, xv, p. 508, February, 1871.

is formed at this stage a large cavity which divides into two parts; the upper part, uniting the hollow tentacles at their base, forms the so-called circular canal, while below it, and connecting with it, we have a large cavity forming the perivisceral cavity, a mode of development of the circular ring and of the perivisceral cavity totally unlike that observed in Ophiurans, Starfishes, Echini and Holothurians.

Metschnikoff compares the mode of development of the upper and lower cavity to analogous processes in the embryonic growth of Alcyonella and other Bryozoa; he traces a striking similarity in the structure and position of the digestive organs and tentacles with similar organs of Bryozoa. However that may be, he has shown conclusively that the larva of Comatula has apparently nothing in common with other Echinoderm larvæ; but we must wait for his figures on this intricate subject before we can decide if the position he assigns to Crinoids is true to nature. — A. Agassiz, in *Amer. Jour. Sci.*

Birds New to Massachusetts Fauna.—I send you the following memoranda, of six species of birds, new to the fauna of this state taken within its limits by myself and friends, with the request that you will publish them in the Naturalist:—

Hudsonian Titmouse (*Parus Hudsonicus*). On October 30th, 1870, I took an adult female at Concord, in company with a few Golden-crested Kinglets (*Regulus satrapa*).

European Ruff (*Philomachus pugnax*). Had a fine specimen sent me in the flesh from Newburyport marshes, May 20th, 1871. Upon dissection it proved a female, with the ovaries so much developed that I judged it would have laid within two or three weeks. This, Prof. Baird informs me, is the sixth that has been taken in America.

Baird's Sandpiper (*Actodromus Bairdii*). A specimen taken on Long Island in Boston harbor, Aug. 27th, 1870, by Mr. H. W. Henshaw of Grantville, Massachusetts. This is I believe the first note of the occurrence of this bird on the Atlantic coast.

Havel's Tern (*Thallaseus Havelli*). A single specimen taken on Ipswich beach by Mr. C. J. Maynard, September 1870.

Marsh Tern (*Geochelidon Anglica*). One taken on Ipswich beach, September, 1871, by Mr. C. J. Maynard.

Barrow's Goldeneye (*Bucephala Islandica*). I obtained an adult

female in the flesh from Cape Cod, December 7th, 1871, which was pronounced by Prof. Baird unquestionably *B. Islandica*. Since then I have seen numbers of females and two fine adult males in the Boston markets, most of them shot within state limits.

Mr. Maynard also informs me that he took two more specimens of Baird's Sparrow (*Centronyx Bairdii*), October 14th and 15th, on the Ipswich sandhills, thereby confirming the hypothesis advanced by him in the "Naturalist's Guide," namely, that they are regular winter visitants from the North.

The Stilt Sandpiper (*Micropalma himantopus*) which I see was recorded in a recent number of the NATURALIST as new to our fauna, I consider by no means rare in its migrations. Indeed, I have seen as many as six or seven sent into Boston market at one time, from Cape Cod, and in the course of a few weeks' shooting in August, at Rye Beach, N. H. (just north of our state limits), secured no less than ten specimens.—WILLIAM BREWSTER, *Cambridge, Mass.*

ERROR IN DARWIN'S ORIGIN OF SPECIES. In the last edition of the above work, p. 149, Mr. Darwin misstates Hyatt and Cope's law of Acceleration and Retardation in the following language:

"There is another possible mode of transition, namely, through the acceleration and retardation of the period of reproduction. This view has lately been insisted on by Prof. Cope and others in the United States. It is now known that some animals are capable of reproduction at a very early age, before they have acquired their perfect characters," etc.

Prof. Cope and others have not insisted on the above proposition, which we imagine to be supported by very few facts. Their theory of acceleration and retardation states; that, while the period of reproductive maturity arrives at nearly the same age or period of the year in most individuals of a single sex and species, the portion of the developmental scale which they traverse in that time, may vary much. That an addition to the series of changes traversed by the parent, would require in another generation, a more rapid growth in respect to the series in question, which is *acceleration*. A falling short of accomplishing that completeness, would result from a slower growth, hence the *process* is termed retardation. Vast numbers of observed facts prove that this is the great *law of variation* towards which little progress has yet been made

by students who are yet chiefly occupied with the coöperative law of natural selection.

Acceleration and retardation of the period of reproduction may possibly have occurred; but the only case in which it has been recognized in connection with the above law, has been in regard to sex. In the human species at least, differences in several characters, mostly metaphysical, seen in the sexes and in certain races, may be consequences of the earlier or later appearance of maturity in this point. — Z.

Parthenogenesis among Lepidoptera.—The Dutch naturalist M. H. Weizenbergh jr. has performed a series of experiments on this interesting subject, the insect placed under observation being *Liparis dispar*, and concludes that it is possible for at least three successive generations to be produced without access of the male to the female. The following are the results of his very careful experiments:—(1) August, 1866, eggs laid by impregnated females; April, 1867, caterpillars appear, and in July perfect butterflies. (2) August, 1867, eggs laid by females of this year are without impregnation; April, 1868, caterpillars appear, and in July perfect butterflies. (3) August, 1868, eggs laid by females of this year without impregnation; April, 1869 caterpillars appear, and in July perfect butterflies. (4) August, 1869, eggs laid by the females of this year without impregnation; April, 1870, no results, the eggs all dried up. The power of reproduction appeared to decrease year by year when impregnation was prevented. Similar results have been noticed in other butterflies, in bees, and notably in aphides.—A. W. B.

Naturalization of Salmonidæ. — The American brook trout, *Salmo fontinalis*, is now thoroughly established in England, where it is the admiration of pisciculturists.

There seems to be no question about the success of the attempt to introduce the European trout, *S. fario*, into the rivers Derwent and Plenty in Tasmania, good-sized fish of that species being occasionally caught there. As to salmon (*S. salar*) and salmon trout (*S. trutta*), the evidence of success is not so conclusive. A letter from Tasmania, dated Dec. 30, 1871, and published in "Land and Water," asserts that there is no doubt that the Derwent is becoming yearly, more and more fully stocked with them, yet admits that no adult salmon have been caught. James A. Youl says that

the salmon and salmon trout have bred in the Plenty ponds, in fresh water, without ever having a chance of emigrating to the sea. Frank Buckland proposes to send out a net, wherewith to test the presence of salmon, from which we may infer that such an obvious means of ascertaining the facts has not yet been tried, although there was ocular proof of the presence of great numbers of fish supposed to belong to the two species in question. The eggs from which the Salmonidæ now in Tasmania, are descended, were sent out packed in ice. A large case of salmoneggs was shipped for New Zealand, last November, packed without ice, in glass jars with damp moss. — C. G. A.

Curious Habits of a Snake. I had for some time living in a Wardian case, a specimen of *Oyclophis æstivus*, received from Ft. Macon, N. C., through the kindness of Dr. Yarrow. The slender form of this snake, and its beautiful green and yellow co'ors have led to the opinion that it is of arboreal or bush-loving habits. It never exhibited such in confinement, however, and instead of climbing over the ferns, etc., lived mostly underground. It had an envious habit of projecting its head and two or three inches of its body above the ground, and holding itself for hours rigidly in a single attitude. In this position it resembled very closely a sprout or shoot of some green succulent plant, and might readily be mistaken for such by small animals. — Edw. D. Cope.

More about Singing Mice. — A correspondent whose name is withheld sends me something that may interest your readers:

Dear Doctor. — You are quite correct, I think, when you say, as reported in the "Sun" of this morning, that the peculiarity of singing mice is not due to a diseased condition. After breeding nearly three hundred white mice with the hope of meeting one of these vocalists I at last succeeded in getting *one* and no more, yet, strange to say, he has never displayed his accomplishment more than three or four times, and he is seven or eight months old. The means I have found to induce him to sing are to keep him in a case deprived of all society and without exercise for several weeks. Upon being placed in a little tin cottage to which is attached a cylinder, he becomes very excited and joyous, and turns round and round twittering his curious note and testifying his pleasure in every possible manner. He is always in perfect health, and the young bred from him are the strongest and largest I have ever produced. He is also tame, very knowing, very pretty, and about as agreeable a companion as a solitary person could desire. I

infer that your mouse is of the ordinary color. I may mention, at the risk of telling you something you know already, that there is a kind of hiccoughing mouse; I also had one of these, but going one day on an exploring expedition in the wilds of a deserted rubbish room, he was lost, night overtook him and he froze to death.

This letter was occasioned by some general remarks made before the Maryland Academy of Science, *à propos* of Dr. Lockwood's recent article, which, like everything else he writes for the NATURALIST, is simply delightful. The whole subject is interesting. Who knows that mice have not *normally* the ability of so modulating the voice as to produce musical notes, though the faculty may be only occasionally exercised? Birds themselves, as a rule, sing chiefly under the special stimulus of the breeding season. True vocal music having been generally, but perhaps too hastily, supposed to be confined to birds more information is desirable. Who can contribute something? — ELLIOTT COUES.

THE MUSIC OF THE RATTLESNAKE. — I have nothing to say in reference to Prof. Shaler's theory of the use of rattles to the snake, but while botanizing over the marshes of Michigan during the past few years, I have had a chance to become familiar with the sound of the rattles of the Massasauga. It is so much like the singing of some grasshoppers that I have often mistaken the sound of the insect for that of the serpent. — W. J. BEAL, *Mich. Agr. College.*

MELANISM. — Noticing what Dr. Wood says, in the last number of the NATURALIST, about "melanism," it occurs to me to say that black woodchucks are found in this region and in Washington, N. H.; and I have had a perfectly black chipmunk from Milford. — SANBORN TENNEY, *Williamstown, Mass.*

GEOLOGY.

GLACIERS IN THE ROCKY MOUNTAINS. — In the NATURALIST for February, in an article on "The Mountains of Colorado, Dr. J. W. Foster denies the existence of any evidence indicating the former presence of glaciers in the Rocky Mountains. He says that so far as he observed none of the rock surfaces are polished and striated, and that those accumulations of sand and gravel in the nature of terminal moraines are entirely wanting.

With regard to the existence of terminal moraines I am con-